Probability Evaluation of Seismic−induced Levee Failure and Contaminant Release in the San Francisco Bay−Delta System during the next 30 Years

Chi Y. Wang

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0190: Probability Evaluation of Seismic−induced Levee Failure and Contaminant Release in the San Francisco Bay−Delta System during the next 30 Years

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The investigators propose to simulate ground motion from a 30−yr earthquake and the geotechnical response (liquefaction) at eight levee and eight contaminated sites in the Bay/Delta region. The objective is to identify 'hot spots' for potential failure and assess how much time is available following an earthquakeinduced liquefaction to avert a potential environmental disaster. The project relies largely on existing geologic, hydrogeologic, seismological and geotechnical data (of the 16 sites, geotechnical information is known to be available at two sites and piezometers will be installed at two sites). The seismic modeling component of the proposed work is very strong, as is the expertise of the associated investigators. The weakness of the proposal is the absence of discussion on (i) the availability and adequacy of hydrogeologic and geotechnical data and (ii) the uncertainty this may introduce into the predicted result. Also not discussed are other sources of uncertainty (e.g. nature of rupture mechanisms or ambient hydrologic conditions), even though probability of failure is cited as a primary objective. It is hard to believe that the "probability" of liquefaction and failure at the modeled sites can be reliably determined without adequate consideration of all these uncertainties. The proposal had three reviews, leading to overall ratings of

fair, very good, and excellent. All reviewers agreed that the topic is timely and important. Two of the reviewers were concerned about the adequacy of seismic, geotechnical, and hydrologic data, which led them to doubt that the predicted results would be ultimately sound. One wrote "Because of the weakness in evaluating geotechnical and geologic conditions, I have serious reservations about the validity of predicting locations of liquefaction" Two reviewers commented on the absence of any plan for knowledge transfer to public or to decision makers. One reviewer's summary is quite apt: "An impressive project in its daring ambition and breadth, but not quite up to it in all details of planned execution, especially when dealing with spatial resolution, availability of sufficient data to parameterize the models, and quantifying the uncertainties and treating probability with the proper rigor. Not quite excellent, but definitely well worth going ahead with it." This reviewer gave the project a "very good". I would opt for a rating right in the middle, but the panel only has four choices. The quality of the seismic modeling and of the investigator's credentials are strong, but the large uncertainties associated with using "representative" geotechnical data diminish the likelihood of a useful product. The proposal does not adequately deal with this issue, so the rating slips to "adequate".

Additional Comments:

The investigators propose to simulate ground motion from a 30−yr earthquake and the geotechnical response (liquefaction) at eight levee and eight contaminated sites in the Bay/Delta region. The objective is to identify 'hot spots' for potential failure and assess how much time is available following an earthquakeinduced liquefaction to avert a potential environmental disaster. The project relies largely on existing geologic, hydrogeologic, seismological and geotechnical data (of the 16 sites, geotechnical information is known to be available at two sites and piezometers will be installed at two sites). The seismic modeling component of the proposed work is very strong, as is the expertise of the associated investigators. The weakness of the proposal is the absence of

discussion on (i) the availability and adequacy of hydrogeologic and geotechnical data and (ii) the uncertainty this may introduce into the predicted result. Also not discussed are other sources of uncertainty (e.g. nature of rupture mechanisms or ambient hydrologic conditions), even though probability of failure is cited as a primary objective. It is hard to believe that the “probability” of liquefaction and failure at the modeled sites can be reliably determined without adequate consideration of all these uncertainties. The proposal had three reviews, leading to overall ratings of fair, very good, and excellent. All reviewers agreed that the topic is timely and important. Two of the reviewers were concerned about the adequacy of seismic, geotechnical, and hydrologic data, which led them to doubt that the predicted results would be ultimately sound. One wrote "Because of the weakness in evaluating geotechnical and geologic conditions, I have serious reservations about the validity of predicting locations of liquefaction" Two reviewers commented on the absence of any plan for knowledge transfer to public or to decision makers. One reviewer’s summary is quite apt: “An impressive project in its daring ambition and breadth, but not quite up to it in all details of planned execution, especially when dealing with spatial resolution, availability of sufficient data to parameterize the models, and quantifying the uncertainties and treating probability with the proper rigor. Not quite excellent, but definitely well worth going ahead with it.” This reviewer gave the project a “very good”. I would opt for a rating right in the middle, but the panel only has four choices. The quality of the seismic modeling and of the investigator’s credentials are strong, but the large uncertainties associated with using "representative" geotechnical data diminish the likelihood of a useful product. The proposal does not adequately deal with this issue, so the rating slips to "adequate".

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The proposal addresses an important topic: the potential impacts of seismically−induced levee failure on all CBDA

ecosystem protection and restoration activities in the San Francisco Bay−Delta. Seismic modeling is the strong suit of this proposal. The research team is very well−qualified to conduct the geophysical modeling activities they propose. The proposed geo−technical element of the project is weak because very little geo−technical information appears to be available. Observations of groundwater and soil properties are limited to only two sites each. “Representative” soil properties will be used at all the other sites. There is no analysis of how the uncertainty from this lack of specific input will propagate through the models and influence the results.

Rating: Adequate

Technical Review #1

proposal title: Probability Evaluation of Seismic−induced Levee Failure and Contaminant Release in the San Francisco Bay−Delta System during the next 30 Years

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Goals and objectives are fairly well stated. Premise is that it is to better use realistic seismic and geotechnical models to develop a probability prediction of where delta levees could fail; this study proposes to develop that failure model. Then using contaminate techniques, the study proposes to determine what contaminations could result if levee failures. Yes, the idea is timely because of the probability of a large earthquake occurring on the San Andreas or Hayward faults within the foreseeable future.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Yes, the authors do a good job of developing the problem that many hazardous waste sites exist around the bay and that many of these sites are founded on liquefiable bay mud. The conceptual model of the problem is well stated, but details of how the study will develop the model are less well expressed. More details describing the study methodology are needed.**

	This study approach will be a full scale implementation of how their model can be used in the 8 critical islands east of Suison Marsh. It is conceivable that if the model is effective in this local area that it could be applied in other areas of the Delta or San Francisco Bay.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Conceptually the approach appears well−designed; however, we are not told enough details about the study methodology to fully judge the approach. The authors propose to develop a ground motion model that incorporates basin effects and this should yield the location where stresses to the levees will be greatest. I believe that the use of both the non−elastic and elastic seismic deformation models will yield new knowledge about seismic focusing that have not been generated before. Details about how their geotechnical data is better are not given. Shallow hand−driven piezometers will be installed as part of this study, to determine the water table and flow directions. This data will help determine liquefaction potential as well as help with contaminate flow determination. I question how realistic the geotechnical input data will be. So based on sparse geologic and geotechnical information presented in the proposal, it is difficult to say how meaningful the model results will be. If the seismic model turns out useful results, this could spur a need to collect better geotechnical information that could be fed into this model. I believe that the contaminate models developed as part of this study will show scenarios; these are not transportable to other areas**

	of the delta because of their site specific nature.
Rating	**good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**These seismic, liquefaction, and flow model approaches are established methods and the authors do a fine job of showing how these methods have worked at other sites and why they should be able to be performed in this location. I believe that the seismic modeling will be successful in showing where seismic stresses will focus in the Delta or Bay. Determining realistic failure locations based on geologic, geotechnical, and groundwater data is more questionable. The study is installing their own piezometers to collect groundwater data, but it is difficult to know if this is enough detail. Without knowing more about the existing groundwater tables and their variation, it is difficult to judge if the number of piezometers to be installed are adequate. We are not told about the depth that the piezometers will be installed, but I question if hammered−in piezometers could go down more than about 10 to 15 feet. It would have been prudent to install a few piezometers deeper, and with a drill rig, in order to get down to at least 30 feet. This could have been useful to see if there is just one aquifer or a series of stacked aquifers. However, I do credit the study for using hammered in piezometers as a useful method to inexpensively obtain shallow groundwater data. My greatest concern with this study is that there is no new geologic or geotechnical data being collected. No drilling was proposed so that subsurface material samples could be collected and described. Along with drilling one could perform SPT tests which are the standard baseline blow count data to determine if material will liquefy. CPT was not proposed either; while CPT is not as accepted of a**

	method to determine liquefaction, per foot of drilling it is much less expensive, and when used in tandem with the SPT it is a highly effective dual technique to get reliable data that extends over a large area. Most certainly the cost of this proposal would have significantly increased if drilling were added, but this reviewer would have preferred a higher study cost that generates reliable data than a lower cost with unreliable data. The hydrologic parameters will be based on pounding in piezometers. Thus, only the shallow aquifers will be evaluated and presumably that will be deep enough.
Rating	**fair**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring of the groundwater is a major effort of this study and is performed through the use of piezometers. The piezometers will be installed during the first year and then monitored for two years. The shallow groundwater depth, the direction of flow, and seasonal affects should all be determined. The proposal has trips to the sites to check the model, but I do not know what will be measured or observed. If there is surface morphology and it will affect levee failure then that may be one parameter to observe. Perhaps sand versus clay can be identified on channel bank exposures, but this was not mentioned as part of the proposal.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Yes, a failure analysis and prediction model will be extremely valuable to planners. Because of the weakness in evaluating geotechnical and geologic conditions, I have serious reservations about the validity of predicting locations of liquefaction failures.**
Rating	**fair**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Authors do not have a record of performance with CALFED. Team−members Wang and Dreger have conducted two research projects on earthquake effects on the Chi−Chi earthquake in Taiwan. The PI and two co−PI's have good and relevant publications related to seismic modeling and earthquake effects in the San Francisco Bay area. Although publications on groundwater modeling were not emphasized, the third team−member−Manja teaches a course on hydrology, so I believe that the team has enough strength in this area. However, the team could use more strength on evaluation of geologic and geotechnical conditions.**

	Study heavily depends on the GSR (Graduate Student Researcher) to perform this work. Of the 7,151 total manhours budgeted to perform this study, 3899 or 54.5% is by this graduate student. While we are given resumes of the PI's, it seemed to be an omission not to include more information about the GSR who will be doing most of the work. At least a resume of the GSR should have been included instead of just one page of emails saying that this student will be coming to school at Berkeley.
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Budget seems small for this amount of field work. It is reasonable, balanced, and matches well with the tasks that it must fulfill.** **For the GSR (Graduate Student Researcher) a percentage of student tuition is included in the budget. Some funding agencies do not allow this, CALFED should verify that student tuition is an appropriate expenditure.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The concept and pertinence of this study are very important. The threat of a large earthquake and liquefaction damage to levees is high. The authors are to be commended for developing a study that assesses this problem. This aspect of the proposal is**

	"Excellent". **However, I placed a “Fair” rating on this proposal because of perceived weaknesses. I believe that the most important product that can be generated is one that shows where seismic focusing can occur– this gets a “Very Good” rating. The geotechnical aspects of this proposal are weak, because they only utilize existing generalized geologic and geotechnical data rather than collect new data or site specific data. Thus, much of the work will involve a synthesis of existing geotechnical data. When a major product of this study is a model that predicts liquefaction location and it is based on weak geologic and geotechnical data – then that affects the validity of the entire model. This part of the study is “Poor”. This weakness also affects the validity of the groundwater contamination results, which on its on merits I rank as “Good”. So overall I give this proposal a “Fair” ranking. The addition of site specific geologic and geotechnical data for the study sites would greatly improve this study.**
Rating	**fair**

Technical Review #2

proposal title: Probability Evaluation of Seismic−induced Levee Failure and Contaminant Release in the San Francisco Bay−Delta System during the next 30 Years

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**Goals and objectives are generally clearly stated, however with two noticeable exceptions: 1) While the proposal states that the project will give the probability of consequences (levee failure, contamination of Delta or Bay waters, perhaps aquifers) it appears that probability seems only considered to the extent that the investigators plan to use scenario events to which the USGS has assigned probabilities of occurrence during a 30−year period. But the remainder of analysis seems devoid of any recognizable probabilistic approach (for details see "Approach"). 2) It is not always clear whether there are sufficient data available to parameterize the problems that the PIs plan to model in 3−D, or whether and how they will have access to the data if they exist (for details see "Feasibility"). Otherwise the ideas are timely and truly important. Results could be most valuable for both planning mitigation measures before the expected events occur, and providing guidance for disaster response operational measures during and after such events.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection

of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**To the best of this reviewers knowledge no such comprehensive effort has been attempted in the study area (perhaps no−where else in world), yet is needed if avoidance of environmental degradation is a serious goal of the operational agencies involved. The project seems to be formulated as if it were almost a full−scale implementation project. Whether it can achieve this depends on two conditions. a) Whether the project can achieve its technical/scientific goals (and with cutting−edge projects, like this one, that is never clear). And b) whether the operating agencies of the targets in question (8 Delta islands, 16 Bay sites) are willing to engage with the PIs (and vice versa) to translate the research findings into operational actions and modifications of past operational procedures and protocols (to the extent that the latter exist in any planned form rather by ad−hoc default). There is no indication (say, by letters of agreement or MOU's) whether the PIs have sought such cooperation in the current stages of their efforts. They should pursue this issue vigorously.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**In a deterministic framework, the study is well designed. There are two types of targets (Delta islands, and contaminated Bay sites), and there are 3 distinct scientific issues to be tackled (scenario earthquakes and related ground motions; soils and levee response regarding ground motion amplification, pore pressure build−up, liquefaction and water**

	release; and hydrologic consequences of delta island inundation and dispersal of contaminants in the river/estuarine bodies of water and in aquifers). This is an extraordinary ambitious set of tasks. Is the approach commensurate with this broad set of objectives? Certainly in terms of PI expertise it seems to be, at least for tackling the three distinct tasks. But in terms of probabilistic methodology the proposal provides no clues. Probabilsitic approach means that for all processes the inherent (random or "aleatory") uncertainties, and all the modeling ("epistemic") uncertainties are fully and systematically accounted for. The latter may be achieved by usage of logic trees or Monte Carlo approaches. The proposal is silent on these matters. To give two examples: 1) multiple rupture scenarios for the earthquakes would need to be considered resulting in different input ground motions for tackling the liquefACTION PROBLEM, due to variations in rupture directivity, AND strength and location of fault asperities etc. 2) Various stream flow levels (representing dry or rainy storm seasons) and tidal conditions will need to be considered that will greatly effect the hydrological consequences, and hence operational responses. Again the proposal is silent on this matter and clearly is not written within a rigorous probabilistic framework. In that sense the contemplated products will not yet be DIRECTLY useful for decision makers, where levels of confidence of the results are important. Rather the work will ultimately CONTRIBUTE to such a framework if followed up in additional work not yet proposed here. The proposed work is very valuable as a necessary but not sufficient step to achieve the ultimately needed outcome.

Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The deterministic approach is well covered as far as methodology is concerned. It is not entirely clear whether the data are collectable or available to do the 3−D parameterization of the seismic, geotechnical and hydrological 3−D models with sufficient spatial resolution. The geologic data (seismic velocities and densities) are needed for basin configuration to model seismic ground motions realistically in this complex environment and for the full seismic frequency range of engineering consequence. Little is said what are the highest frequencies that the ground motion modeling will attempt. The proposal is not clear on how detailed the geotechnical coverage is at all the sites to be evaluated. And if coverage is not suffcient, how proxies are derived for parameterization, not so much for the SHAKE (or similar) computations of site response. But this is especially true when it comes to consider more complex soil rheologies and non−linear behavior with modeling the pore−pressure build−up, right up to liquefaction, water extrusion, and/or ground and levee failure. If geotechnical data are sparse this leads once again to the question what are the uncertainties in the modeling procedure, and what is the level of confidence one can have in the results to mimic reality? This is stating the obvious, and I wish the PI's would have exactly done that in their proposal. They seem confident in what they want to do, which is great, but do not document the wise thoughts they may (or may not) have given to how feasible it is to do the modeling, and to quantifying the uncertainties in such a way that the outcome will be credible and, hence, useful. The PIs are on the right track in the**

	basics, but have not expressed the uncertainties that lie ahead, and what may be needed to address them and overcome related probabilistic issues.
Rating	**good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring is restricted (within this project) to piezometric measurements at the selected sites. It only can be hoped that at least some of these sites are co−located with strong−motion instruments (both laterally and down−hole). Have the USGS or CAGS strong−motion programs any instruments (or vertical arrays) at and below any of the targeted levees and other target sites? The project, more by necessity than choice, will largely rely on seismic data from existing installations, unless The PIs can influence these two agencies to place additional instruments suitable for this study. The proposal seems to provide all contingencies to analyze the collected or supplied data from the proposed monitoring devices.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The contemplated products will be clearly valuable (with the caveats earlier stated). There is no explicit mentioning how the**

	project−collected and derived data (results) will be integrated in any existing or new public data dissemination or management system. This needs some thought. In addition, there will be clearly interpretive outcomes from the project (whether they are directly useful for operational applications needs to be seen). These results should be formulated together with operational agencies for the target sites to bring these agencies into the fold early in the course of the project.
Rating	**very good**

Additional Comments

Comments	**The above comments may sometimes sound highly critical. They are, but only in a constructive sense by trying to envision all the possible roadblocks that an ambitious and comprehensive study like this may encounter. I believe that the PIs are on the right track of an important issue that sooner or later will haunt the Delta and Bay area communities and operators, as outlined by this proposal. The sooner the issues are tackled the better because it will take time and efforts that probably will go beyond what is proposed here to really address solutions, including the costs it may take to actually implement remedial engineering projects. But this is clearly beyond the scope of this valuable trail−blazing research project. I feel strongly that one research aspect may need to be added. I suggest the PI's consider the relative vertical motions from sea level rise (from global warming) and from tectonic land subsidence, whether co−seismic, inter−seismic, or from sediment compaction and/or man−made fluid extraction or injection (if and where applicable). These processes are almost certain to aggravate the**

	hazards and risks described in the proposal, and will do so at ever increasing rates during the forthcoming century.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The team of three PI's, amended by a prospective graduate student with a well suited background and education, seems fully up to the task. The one experience that is not explicitly covered has to do with dealing with probabilistic methods and uncertainty. It could be a special assignment to the graduate student to get herself trained in these issues (Norm Abrahamson is an excellent resource in the Department) to fill this possible gap during the course of the project.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget seems reasonable, justified and sufficiently detailed. It is certainly reasonable given the comprehensiveness of the project in its threefold disciplinary approach and two sets of targets. In fact, given its ambition, the project is a bargain.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	

	An impressive project in its daring ambition and breadth, but not quite up to it in all details of planned execution, especially when dealing with spatial resolution, availability of sufficient data to parameterize the models, and quantify the uncertainties and treat probability with the proper rigor. Not quite excellent, but definitely well worth going ahead with it.
Rating	**very good**

Technical Review #3

proposal title: Probability Evaluation of Seismic−induced Levee Failure and Contaminant Release in the San Francisco Bay−Delta System during the next 30 Years

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals and objectives are clearly stated in the beginning: To carry out a rational and quantitative evaluation of the probability of seismic?induced levee failure and contaminant release in the San Francisco Bay?Delta System in the next 30 years; To identify the 'hot spots' in the Bay−Delta's water system under seismic loading, thus to provide policy− and decision−makers a rational and quantitative basis to assess related water management issues and to develop contingency plans for emergency measures prior to the occurrence of such events.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is highly relevant to the State of California and the Bay Area in particular due to the high probability of large earthquakes, the high proportion of unconsolidated sediments in populated regions, and the high level of soil contamination. The PIs have laid out the importance and justification for the**

	study based on these points.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Many pieces of the puzzle exist in the form of geotechnical data, seismic models and groundwater models, but this study aims to integrate these components into a comprehensive evaluation of risk related to seismic−induced levee failure. Integrating the groundwater simulations with the seismic levee failure results provides an additional benefit to this work in that it looks at the unique groundwater contamination risks associated with specific documented pollution sources in the region. Their approach combines the strengths of each PI and state−of−the−art computer simulations to achieve the project goals. I have little doubt that the project will generate important and novel new information that will be very useful to decision makers.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The project is feasible, with certain caveats that are fully disclosed in the proposal. In particular, the linear method, while relatively straightforward, is limited by it oversimplified assumptions to the point where it is marginally useful. It may be useful to constrain boundary limits for the more robust**

	non−linear treatment, which while able to better represent real systems, is hampered by the need for very precise and well−defined input parameters, which may not exist in the geotechnical input data. Having said this, it is clear that the PIs recognize the strengths and limitations of each approach and have presented a methodology that is quite feasible.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The Task Form lays out a detailed account of the plans for monitoring ground water parameters, performing field tests, and developing numerical simulations. Semi−annual reports are scheduled to monitor progress throughout the project. No problems foreseen here.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	All of the products from this study are discussed. Diagramatic deliverables will be of use to decision makers; the integrated database will be very valuable for improving models and expanding the regional scope later. The deliverables are: 1. An integrated database for the existing geologic, hydrogeologic, seismological and geotechnical data for the San Francisco Bay−Delta System. 2. Model hydrographs for groundwater flow at the 16 selected sites. 3. Diagrams of simulated wavefields from scenario earthquakes on each of the active faults in the San Francisco Bay

	Region. 4. Diagrams of simulated seismic response, pore−pressure change, liquefaction and levee failure (if any) at the 16 selected sites. 5. A list of the critical sites where simulated levee failures or contaminant releases (if any) have occurred. 6. Quantitative estimates of the probability of levee failure or contaminant release at the San Francisco Bay−Delta System in the next 30 years. 7. Diagrams of simulated contaminant migration and breakthrough curves from each liquefied site (if any) to local streams and open waters. 8. Reports of findings, diagrams for illustration, and posters for presentation.
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The PI has extensive experience in this kind of work over the past 40 years, and the co−PIs are highly respected young scientists who have worked with the PI on related research. Very high caliber scientists all.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is realistic.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is a well−conceived project with a high likelihood of success that is being conducted by highly capable and qualified scientists. The products will be useful to both the scientific community and to decision−makers, and a graduate student will receive valuable training and a degree from this project. I give it an excellent rating without hesitation.**
Rating	**excellent**